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ABSTRACT

In this review of Donald A. Erickson's book, "Educational Organization and Administration," the author focuses on what Erickson calls a "paradigm shift" in research and in the conceptualizations that undergird research. Erickson argues that research should be concerned primarily with productivity in the educational system, and he views productivity as student outcomes (or achievement). The reviewer takes issue with Erickson's position that only conditions that make a difference in student outcomes are appropriate for research. He asserts instead that organizational structure and process, even though these elements may not be directly related to student achievement, are equally valid areas for researchers to investigate. (Author/DS)

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A Critical Review of Erickson's Readings
in Educational Organization and Administration*

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A CRITICAL REVIEW OF ERICKSON'S READINGS
IN EDUCATIONAL ORGANIZATION AND ADMINISTRATION

W. W. Charters, Jr.

Books of readings sponsored by professional associations are often used to open new fields of study and to promote new lines of inquiry, not just to offer handy resource materials for college course, and this was clearly the intention of the AERA series on educational research.

Erickson took the mandate seriously. He chose to prepare this volume not to reflect the state of the field as it presently is--a state that he apparently deplores--but to specify a direction in which he believes research on educational organization and administration should move. To use his phrase, he would like to induce a paradigm shift in research and in the conceptualizations that undergird the research. He would like investigation to focus on the practical and the policy relevant.

Although Erickson spends a good bit of time explicating the thesis, I am not entirely certain what it all means. It is not firmly fixed in my mind what Erickson would have researchers do differently, after attending the thesis, from what they had been doing before. Let me look at the "paradigm shift" more closely, first in regard to the criticism it implies of the current state of affairs and then with regard to the direction Erickson urges us to pursue in the future.

Perhaps first we should dismiss the term "paradigm shift," at least in its literal sense. The phrase suggests that there is a prevailing paradigm in the field--that there is something to shift from. Erickson would be the last to argue that the field of educational organization and administration is dominated by a single, tight theoretical system within which its research

questions are phrased. The problem may be precisely the reverse. Erickson would surely attribute his discomfort with much of the research to the absence of any underlying conceptual framework. It is not a matter of paradigm shift with which he is concerned as much as establishing one in the first place.

What is it about current research trends that Erickson finds objectionable? This is singularly difficult to answer once we get beyond the general misgivings we all harbor about work in our field. He decries, for instance, "the frequent preoccupation of scholars with esoteric bypaths that have neither practical nor theoretical significance." Of course. Most everyone deplores research without significance. He has particular misgivings about the spate of studies of administrative behavior, teacher morale, organizational climate, and "a score" of similar concepts, which he finds difficult to synthesize in any meaningful way. I think we all would share his concern over the endless, witless administrations of the LBDQ, OCDQ, POS, ABC, and XYZ scales to haphazard collections of teachers and administrators. Erickson is also sensitive to the methodological shortcomings of our research--the enormous hazards to causal inference in studies of an ex-post-facto design, even with the application of path analytic techniques, the nongeneralizability of in-depth case studies, the weakness of proxy measures far removed from the concepts they are supposed to index, the desperate need for replication of key studies, and so on. These points are well taken, but they are not matters that a new paradigm of any variety is likely to solve.

Somewhat more pointed is the criticism implied by Erickson's call for a focus on policy-relevant research, for attention to variables that can be

manipulated. By implication current research, or much of it, is irrelevant for policy making. He does not linger on the issue long enough, though, for one to know the sorts of investigation he has in mind, although he offers a few cues when he begins to expand on the theme. Scholars of today, Erickson believes, have a "deep-seated fixation on organizational process and structure in their own right" (his words), a matter he lays to our preoccupation with disciplinary concerns. He writes,

It is widely assumed that our task as intellectuals is to use schools and other educational settings as handy laboratories for testing, revision, and elaboration of conceptual material valued by social psychologists, sociologists, anthropologists, linguists, economists, and political scientists. Many researchers, similarly, seem part of an interdisciplinary effort to understand organization and administration in the abstract, with little regard for such specific settings as education (p. 3).

The cues I get from this and similar passages about what we have been doing wrong in Erickson's view are sufficiently oblique that they can be shrugged off as applying to someone else's investigations but not my own. Who that "someone else" is remains unspecified.

Still and all, the cues leave me vaguely uneasy. Maybe it is me he is talking about. It is true that I am vitally concerned with organizational process and structure, although I would not necessarily call it a "deep-seated fixation." I certainly am interested in applying, testing, and elaborating sociological and social psychological theories as I try to understand what schools and similar institutions are all about, and I do happen to believe there are some universal processes that apply in many settings if seen in sufficiently abstract terms. Perhaps Erickson has a message for me and my

like-minded colleagues. To get a better fix on it, though, we must turn to Erickson's discussion of the form of the research we should be doing instead.

The new paradigm is not complicated. (Professor Nault was able to capture its essence in two sentences.) Research in our field, Erickson says, should be formulated in view of a causal model that connects organizational process and structure to criteria of organizational goal achievement; we should be developing theories of organizational productivity. To quote:

In the future, hopefully, persistent attempts will be made to discover what organizational arrangements are linked to school outcomes, and those outcome-relevant organizational arrangements will be singled out for intensive study (p. 126).

That is, once we know the things about schools that make a difference for student learning we can get busy and study the leverage points for modifying them. Continuing to quote,

The development of a body of empirically verified theory concerning the internal and exogenous factors that govern vital phases of educational systems. . . may prepare us to take informed action as findings emerge to link these phases more precisely and predictably to the outcomes we wish to maximize (p. 126).

It is in this way that our work will become practice-oriented and policy-relevant.

A central problem in all this, of course, is in specifying which phases of the educational system are vital and which ones are not, in determining the particular aspects of classrooms, schools, and school districts that have a reasonably high probability of affecting goal achievement, for those are the areas where we will confine our research.

Some of the work in our field, Erickson believes, should be targeted directly to this question. We should not wait until the educational psychol-

ogists and instructional and curricular specialists establish the connections between cognitive learning and teaching materials, grouping practices, or instructional behavior, but we should get into the fray ourselves. We should not count on the educational sociologists and social psychologists to catch all of the social influences on academic achievement. Erickson seems to feel that we students of educational administration can pick up facets that they will miss if we, or at least some of us, cultivate the kind of expertise necessary to become students of the learning process.

Other workers in the field, though, should concentrate on understanding and explaining the organizational phenomena themselves--the vital ones in particular. Erickson says that, while some studies should be concerned with the ways students respond to organizational conditions in the schools, others "should focus largely on the . . . variables that can be manipulated to produce those organizational consequences" (p. 2).

My vague uneasiness that our past research has been entirely out of keeping with the Erickson paradigm begins to subside a bit with this passage. It is a little clearer how he would have us change our ways. There is nothing wrong, it now appears, with an interest in organizational process and structure in their own right, so long as our studies are more than mindless inspections of associations among sets of variables (and there have been plenty of such atheoretical correlations) and so long as we attend, in our theorizing and data collection, to determinants that can be manipulated by policy makers. Much of the trouble with the past research in Erickson's view, apart from the nontheoretical character of some of it, lies in the choice of organizational

phenomena taken as the object of the study. They have not been chosen with an eye to their vitalness to student outcomes.

So what are these vital organizational phenomena to which we should switch our research attention? That is what the first half of the book is about. Here are selections that present evidence and speculation regarding the impact of student social relationships, the allocation of various school resources, and other structural conditions on such outcomes as academic achievement, college aspirations, and student commitment to the educational enterprise. They contain a wealth of ideas of organizational conditions we should understand more fully in their own right in order to know how to apply levers for greater productivity. (It is these selections, incidentally, that give the book its appearance ~~of~~ a reader in educational sociology. The first half is.)

Thus, Erickson offers his notions of the leverage points. Other researchers may have different notions, but his approach to the matter is clear. To him, the instructional competence of teachers comes out as a big factor in goal attainment, and we should know a good bit more about their recruitment, retention, motivation, and allocation to students. Studies of teacher morale, though, should be abandoned, since morale has no demonstrated relevance to student outcomes. Similarly, investigations into administrator behavior, interpersonal orientations, decision-making styles, and the like are not very important in Erickson's scheme of things, since he finds no trustworthy evidence that the school executive makes much of a difference *to student achievement.*

The paradigm, then, consists of a criterion by which to select problems for study, conditions that make a difference for student outcomes, and a

directive concerning the variables to include in our study of the conditions, viz., those that can be manipulated by administrators.

The proposal has a certain appeal to it. I am sorry Erickson did not develop more thoroughly the nodes and articulations of the causal chain he envisages running between school inputs and organizational outputs. It surely must be a long and intricate one with innumerable proximal connections worthy of detailed investigation, regardless of their ultimate link-ups with goal achievement indicators or even with the vital organizational conditions that promote goal achievement. A book of readings may not be the appropriate place for such a theoretical exposition by an editor, but had Erickson undertaken it, he might have found that some of these proximal links have been the center of research attention in the field for a good many years. He might better have specified, for instance, how the past research on administrative behavior has gone wrong and where it should go next. As it stands, the proposal treats the second-order problems, the domain of organizational structure and process, as a "black box," or at least a mostly opaque one. It is for this reason that I cannot get firmly in mind how much of a departure from our past work the new paradigm implies.

The matter of understanding its specific meaning, however, looms less as a disturbing element of the proposal for me than its underlying spirit. I have reservations about any call that would lock an entire field into a single programmatic formulation, whether offered by Erickson, the National Institute of Education, Charters, or anyone else. I have especially grave reservations when the focus is so narrow and constraining. The focus on

student outcomes, school productivity, or organizational goal achievement-- take your pick--is seriously constraining. I am not quibbling, mind you, with the fact that measured cognitive outcomes, which "goal achievement" usually boils down to, are narrower than intended outcomes nor with the fact that affective outcomes are usually ^{given} short-shrifted in the process of operational definition. My concern is broader than that.

For one thing, the single-minded focus on the manifest goals of the educational institution ignores what may be the most important contribution of an organization theory perspective to our thoughtways about schools and other collaborative enterprises, viz., the cruciality of the system maintenance function of the organizations. From Chester Barnard on down to the present writers we have learned that enterprises of more than momentary duration must divert resources from present productivity in order to secure productivity in the future. For a school system to be productive next year, there must be a school system. In the short run, at least, the two functions of goal achievement and system maintenance interfere with one another, and the most challenging problems of policy and management are those of achieving a long-run balance of the two.

That the proposal distracts attention from system maintenance concerns is revealed in the following passage where Erickson elaborates on his theme that our research should be practical for administrators and their training. He says,

The assumption in much research and training seems to be that theoretical ferreting into group and organizational phenomena is sure to make the educational leader more effective. On the contrary, if the school administrator is to be more than a human

relations oilcan, fiscal funnel, or public relations pitchperson, he/she needs theory concerned with school productivity. Many organizational arrangements can be devised that might keep the money flowing, the community mollified, and the staff harmonious and content; but which of these, if any, will facilitate goal achievement? . . . From where I stand, executives-in-training can learn much more from a school superintendent like James Redmond than from Talcott Parsons (p. 4).

Granted that it is difficult to decipher Parsons without going to the trouble of translating him into German first, I wager that Erickson would hear Redmond emphasizing, as does Parsons, the cruciality of the system maintenance function if he listened intently. The executive must be a "human relations oilcan," a "fiscal funnel," and a "public relations pitchperson" if the school is to be in any shape to graduate the next generation of seniors, and it is a distinct disservice to belittle these operations by demeaning word choice or to hinge their importance on demonstrated correlations with student outcome measures. If Erickson is looking for "practical" issues in administrator training, they lie in the dilemmas the practicing administrator daily confronts concerning the trade-offs between goal achievement and system maintenance.

A focus on organizational productivity is narrow in another and more disturbing way. Scholars of educational administration are asked to become high level technicians. Ours would become a field of technology rather than inquiry. Erickson's stress on the essentiality of conceiving of educational organization and administration as an applied field seems to carry these implications rather overtly. Means-ends connections are what count, and the ends are those as defined by established members of institutional sector we serve. It is as though the work in our field were governed by this directive from

educational policy makers: "Here's where we want to get. Your job, ladies and gentlemen of the research community, is to tell us how to get from here to there, and don't ask any foolish or embarrassing questions along the way." Like, don't ask these policy makers about what it is they intend to produce, and certainly don't raise foolish questions of them about how they got to be policy makers in the first place and what they get out of policy making or how these things might have a bearing on productivity. These are none of our business.

I exaggerate, but it is true that students of an applied field constantly risk cooptation by the sponsoring institution or occupation, even without special urging. It is too easy, as it is, to accept as our own the practitioners' specifications of the researchable questions. It is all too easy to adopt their thought categories, definitions of the important and unimportant, their tacit assumptions regarding the situational givens, and so on, failing to recognize that these are products of their particular social location and vested interests in that location. Not that our modes of formulating questions for research would be better, or free of similar constraints, but they are more likely to be disciplined, open to inspection, and subjected to empirical test. And they do offer practitioners an alternative perspective from which they may view their world. I believe practitioners count on such alternative perspectives from our field of scholarship, except when we offer them without humility or reservation.

Finally, ^{2nd} most disturbing of all, the proposal to fasten on organizational productivity requires us to view the people who inhabit the school in purely instrumental terms. A great sector of the American public [above and below the 49th parallel, that is] live out much of their lives in association with the educational institution--as teachers, students, administrators, school trustees, and on and on.

Except for the family it is the institution that actively engages the most members of our society. Are we to shut from our purview of researchable questions all aspects of this life save those that bear directly on goal achievement? Are we to turn our backs on an understanding of the conflicts and rewards of social living in the educational setting, the hazards and prospects of personal gratification, the opportunities it provides for personal and social fulfillment, independently of the instrumentality to higher test scores? In short, must we always regard the people in the school setting as means to an end and never as ends in themselves? I think the spirit of the paradigm says so, and I think it is intolerably short-sighted.

The call to make educational organization and administration a field of technology rather than one of inquiry reinforces the already strong inclination to phrase "how to" questions rather than "why" questions. We are already under duress to feature prediction over explanation, pragmatics over theory. Erickson, personally, is too sophisticated to make these terms into polar opposites, but that is not true of all of our publics, benefactors, or even members. He should be as nervous as I with the prospect that the "paradigm shift" would have such an unintended, if not unanticipated, consequence.

I would like to close on a rather different note. I do not envy the task Erickson undertook in agreeing to pull together the exemplary work in our field, and I am in no way prepared to say how I would have done it differently. He was confronted by acres of disjointed, theoretically barren, noncumulative, and downright shoddy studies that wove no strands or themes.

To be frank, I suspect that Erickson's proposal of a paradigm was less a call for new research directions in the future than an ingenious solution to his problem as an editor: how to compile the work in this disarrayed field of ours in an intellectually satisfying fashion. Seen in this light--as a book of stimulating new ideas and intellectual integrity--it is a masterful job.

I would especially like to call attention to a feature of the book that is bound to escape your notice if you look only at the table of contents of the reading selections. A full one-eighth of text represents Erickson's own contribution. The contribution consists of detailed commentaries on the individual selections and, above all, the provision of context in which they fit. Generous reference is made to related research and theoretical positions both in and out of our field, revealing Erickson's amazing command of the literature. This contribution is worth half of the price of the book.